

*B. F. SKINNER'S SCIENCE AND HUMAN BEHAVIOR: ITS ANTECEDENTS
AND ITS CONSEQUENCES*

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Skinner's *Science and Human Behavior* marked a transition from a treatment of behavior that took physics as its reference science to one that emphasized behavior as a fundamental part of the subject matter of biology. The book includes what may be Skinner's earliest statement about the similarity of operant selection to Darwinian natural selection in phylogeny. Other major topics discussed in the book included multiple causation, private events, the self, and social contingencies. Among the important antecedents were Skinner's own *Behavior of Organisms* and Keller & Schoenfeld's *Principles of Psychology*. Current developments in education, behavioral economics, and some behavior therapies can be attributed at least in part to Skinner's seminal work. The effective behavioral analysis of governmental and religious systems will probably depend on elaborations of our understanding of verbal behavior.

Key words: Skinner, *Science and Human Behavior*, *Behavior of Organisms*, Darwin, natural selection, selection (of behavior), human behavior

In anticipation of the controversy that his book on natural selection was to provoke, Darwin limited his comments on whether his theory was applicable to humans to just a few lines. In the last chapter of *The Origin of Species*, he wrote: "It may be asked how far I extend the doctrine of the modification of species" (Darwin, 1859, p. 483). When he continued on the next page that "I should infer from analogy that probably all the organic beings which have ever lived on this earth have descended from some one primordial form, into which life was first breathed," he did not mention humans explicitly. But it was obvious that he had not excluded them. A few paragraphs later he went on: "In the distant future I see open fields for far more important researches. Psychology will be based on a new foundation, that of the necessary acquirement of each mental power and capacity by gradation. Light will be thrown on the origin of man and his history." It was only in his *Descent of Man* (Darwin, 1871) that he took on the issue without compromise and in considerable detail.

I am indebted to my colleague, Robert R. Provine, for the observation that organisms must "spond" before they can respond. Though his usage was coined in reference to movements of the embryo, it is appropriate to the emitted units of operant behavior.

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In the final chapter of his *Behavior of Organisms* (Skinner, 1938), Skinner too wrote of the relevance to humans of principles derived from the study of nonhuman organisms: "The reader will have noticed that almost no extension to human behavior is made or suggested. This does not mean that he is expected to be interested in the behavior of the rat for its own sake. The importance of a science of behavior derives largely from the possibility of an eventual extension to human affairs" (p. 441); "The careful reader should be as able to make applications as the writer. . . . Let him extrapolate who will" (p. 442). Later he continued, "I may say that the only differences I expect to see revealed between the behavior of the rat and man (aside from enormous differences of complexity) lie in the field of verbal behavior" (p. 442). The book in which he explicitly took up the human implications was *Science and Human Behavior* (Skinner, 1953).

ANTECEDENTS

Behavior of Organisms was replete with data, especially in the form of cumulative records. It began with one chapter on terminology and systematic issues and another on methodology, and then presented a wealth of data on reinforcement, extinction, schedules, discrimination, differentiation, drive and a variety other phenomena. A chapter on behavior and the nervous system was followed by a brief concluding chapter. In its overall struc-

ture and its emphasis on the priority of data, the book resembled such classics as *The Integrative Action of the Nervous System* (Sherrington, 1906), *Behavior of the Lower Organisms* (Jennings, 1906), *Animal Intelligence* (Thorndike, 1911), and *Conditioned Reflexes* (Pavlov, 1927). Taken together, all were major antecedents of the book whose 50th anniversary we celebrate here.

Science and Human Behavior was written as an introductory undergraduate textbook in psychology especially for use in Skinner's Natural Sciences 114 course at Harvard. One more of its antecedents was another undergraduate textbook, *Principles of Psychology* (Keller & Schoenfeld, 1950). Keller and Schoenfeld (K&S) was rich in data, though drawn from a wide range of psychology authors and journals rather than from one laboratory. When I enrolled in Introductory Psychology at Columbia in fall 1954, it was a 1-year laboratory course with K&S as its textbook. The next year I took a course in experimental psychology that included Skinner's *Behavior of Organisms* as one of the required texts (Catania, 1988). *Science and Human Behavior* was fairly new and often commented upon, and I and some other students read it independently of the course assignments (my copy is from the second printing, dated 1956).

In his autobiography, Skinner (1983) commented on the overlap between his book and K&S: "In March 1951, I sent Fred [Keller] three hundred mimeographed pages of *Science and Human Behavior*. To some extent they covered the same material as the Keller and Schoenfeld text, but, as I explained, 'I don't see how I can write the latter half of the book without at least this much factual buildup'" (p. 44).

How different the coverage was. Where K&S had provided illustrations and data and the scholarly apparatus of references and notes, Skinner's book included none of those. It was unconcerned with antecedents and therefore presented itself more as a break with the past than as an outgrowth of it. Historical links were most evident in the vocabulary, and those would be obvious mainly to readers with a sophisticated background in the field. For example, Skinner continued to apply the term *conditioning* and its derivatives to operant behavior as well as to respon-

dent behavior, though less frequently than in earlier work (in spite of the pedagogical disadvantages, some diehards still persist in that dual application of the term). But along with the dramatic stylistic changes came the profound shift from a perspective anchored in the physical sciences to one organically rooted in biology:

In both operant conditioning and the evolutionary selection of behavioral characteristics, consequences alter future probability. Reflexes and other innate patterns of behavior evolve because they increase the chances of survival of the *species*. Operants grow strong because they are followed by important consequences in the life of the *individual*. (Skinner, 1953, p. 90)

This quotation is one of B. F. Skinner's earliest references to the analogy between phylogenetic and operant or ontogenic selection and is sometimes missed. In mentioning Skinner's move toward selection, for example, Baum (2002) cites only the much later *Science* paper of 1981. The quotation marks a transition from a treatment of behavior that took physics as its reference science to one that emphasized behavior as a fundamental part of the subject matter of biology. Biology is a historical science, in the sense that it deals with the particulars of life as it has evolved here on earth. The evolution of the various species on our planet would have followed a totally different course, for example, had our history of impacts with extraterrestrial bodies been different. It is therefore an open question whether the scientific methods that evolved in the service of the physical sciences are well suited to addressing the issues of life and behavior that are the concern of the historical sciences of biology and behavior.

The learning theories of the 1930s formulated laws and derived principles from those laws. Like other learning theorists of the time, Skinner built such formal structures into his early work (e.g., his *Laws of the Reflex*: Skinner, 1938, pp. 12–33). But though they could be built into theories, they were not foundations to be built upon. In *Science and Human Behavior*, Skinner (1953) dropped the formal structures and made the ties to biology explicit in his many references to evolutionary contingencies.

Those ties were to be strengthened in his later writings (Skinner, 1966, 1975, 1981,

1984). For example, his treatment of phylogenetic selection shifted from the selection of species to the selection of organs and organ systems (Catania & Harnad, 1988), and his elaborations on the nature of the units that were selected represented an integration of selection with some of his earliest concerns about behavioral units (Skinner, 1935). Throughout, he was clear about the ways in which phylogenetic and operant selection supplemented each other; contrary to common opinion, especially outside of behavior analysis, he did not argue that operant selection in some sense superseded phylogenetic selection. Rather, he argued that the possibility of operant selection could come about only as a product of phylogenetic history.

Darwinian natural selection is the most familiar type of selection, but other types include operant or ontogenic selection of behavior by its consequences and the cultural selection of patterns of behavior as they are passed on from one organism to another. Parallels among the types of selection have been explored in considerable detail (Catania, 1978, 1987b, 1995b; Skinner, 1981, 1984; Smith, 1986). Cultural selection has much in common with memetic selection or the selection of memes in Dawkins (1976). For Dawkins, a meme is a replicating entity that can pass more or less easily from one brain to another (a catchy tune is one example). But cultural selection is much more specific about what is selected: It is behavior itself.

Skinner regarded selection as a causal mode that differed from those historically recognized in the physical sciences (Skinner, 1981). An answer to a question of causality that appeals to a history of selection is different from one that appeals to a more limited and usually more local sequence of historical events, even if the sequence involves multiple causes. Some accounts of selection seem instead to be attempts to reduce it to more traditional causal modes. Hull, Langman, & Glenn (2001), for example, provided their interpretation in the course of comparing phylogenetic and ontogenic selection with selection in immunological systems while they rejected cultural and memetic selection as viable examples of selection (cf. Catania, 2001). They also argued that a mechanism for replication must be specified. Yet Darwin was able to characterize essential properties

of natural and artificial selection even though he had available only very limited information about what we now call genetics. Knowing the mechanism for replication of the units that are selected is not a prerequisite for identifying a selective system (though it is, of course, nice to know).

Each type of selection necessarily involves variations that provide the source materials upon which it operates and some basis for selecting what survives (cf. Neuringer, 2002). In ontogenic selection, for example, responses are affected by their consequences within the lifetime of the individual organism, as when, with a food-deprived organism, responses that produce food continue to occur and others do not. Here the opportunity to eat is a consequence that selects some responses and not others, in the sense that the responses that produce food survive and other responses extinguish.

OCCASIONED BEHAVIOR

Many implications of selection were only hinted at in *Science and Human Behavior* and were to be developed later. The book was also innovative in other ways, however. After a brief section on the possibility of a science of human behavior, a major section of 11 chapters was devoted to outlining the basic principles of behavior analysis. The distinction between function and topography and the concept of multiple causation were examined in detail and shaping provided examples of operant selection at work (though more attention seemed to be given to other phenomena such as the behavior transiently maintained by accidental contiguities of responses and reinforcers in the phenomenon that Skinner referred to as superstition).

Colloquial usage implies that responses are reactions *to* something, but for Skinner, operant behavior was emitted (would instances of an operant class have been better called "sponses?"). Furthermore, a stimulus did not elicit these types of responses; it could only occasion them. And motivational conditions and emotions were to be understood in terms of environmental setting events as opposed to internal states. These alone were enough to engender what we might now call a culture shock in some of Skinner's readers.

If these culture shocks were not enough, next came the section on the individual as a

whole. Here was the consideration of self-control as the competition between short-term and long-term contingencies involving consequences of different magnitude, of thinking as covert behavior, of reports of private events as verbal behavior shaped by verbal communities that had access only to public correlates, and of selves as functionally organized systems of responses. It didn't end there for next came a section on social behavior with chapters on personal and group control, and then one on controlling agencies such as governments and religions and schools, and finally one on control and the design of cultures. The last topic (p. 430) made it clear that Skinner's selectionist view was yet to be refined: Later he would speak of the survival of particular patterns of behavior in cultural selection rather than of the survival of cultures. Nevertheless, there was hardly a significant aspect of human endeavor that was not captured in one way or another by the net that Skinner had cast so widely. As noted by Verplanck, "Skinner's specified area of interest is all the (lawful) behavior of all organisms in all environments" (Verplanck, 1954, p. 270).

In 1979, an editor asked me to review a submitted article that proposed a distinction between Skinner's earlier and later behaviorism. The author contrasted an initial position characterized by the early experimental papers and *Behavior of Organisms* with a later position now recognized as contemporary radical behaviorism. The arguments emphasized Skinner's treatment of private events rather than his treatment of operant selection. I wrote a sympathetic review in which I recommended acceptance with revision, focusing mainly on some historical points. For example, the author had characterized Skinner's early work as positivistic or operational but, notwithstanding the important role of Mach (e.g., 1914) in Skinner's intellectual history, there were too many lines of evidence to the contrary. Not least was that, despite its title, "The Operational Analysis of Psychological Terms" (Skinner, 1945) was a renunciation rather than a defense of operationism.

The argument for two Skinners was nevertheless intriguing. Day (1969) had already pointed out parallels between Skinner's position and Wittgenstein's, so the comparison

of Skinner's writings and Wittgenstein's was especially compelling. Wittgenstein's (1922) *Tractatus Logico-Philosophicus* was to Skinner's (1938) *Behavior of Organisms* as Wittgenstein's (1953) *Philosophical Investigations* was to Skinner's (1953) *Science and Human Behavior* and his subsequent *Verbal Behavior* (Skinner, 1957). In both cases, the earlier works were highly technical and tightly structured, and the later were far freer in both style and substance. Wittgenstein's *Tractatus* was written at least in part as a reaction to the Vienna Circle and its evolving versions of logical positivism. Skinner's *Behavior of Organisms* was written in the context of competing behavioral systems such as Hull's (1943). Thus the operant enterprise was temporarily sidetracked by the attempt to construct a systematic edifice of behavioral laws buttressed by paradigmatic formulas consistent with models from the physical sciences. Both Skinner's and Wittgenstein's formal systems could easily be regarded as detours.

In any case, I wrote my review without knowing who the other reviewers might be. In it I remarked: "That some of Skinner's recent statements should contradict some of the things he said four decades ago should not be a surprise. Yet the failure to note the changes has often led to misunderstandings. . . . Thus, publication of this paper would not only represent a contribution to the history of behaviorism, but might also serve the valuable function of leading others (perhaps even Skinner himself?) to deal explicitly with the issue." With regard to Skinner on private events and the philosophy of science, I went on to say:

Skinner has more and more been substituting a behavioral treatment for the traditional language of science. Thus, in this view inference itself is a kind of behavior, and is not to be used as a basis for classifying kinds of events (probably because inference has not itself been well analyzed). Ergo, we should not worry about whether or not private events are classified as inferred, because inference is not yet (and perhaps will not become) a dimension appropriate to a behavioral classification. . . . The issues are not simply those of accessibility and verification (which vanish along with the discarded operationism). If that were all, then for a solitary observer in a distant forest, hearing the sound of a falling tree would be as private as a toothache. For Skin-

ner, however, there are important differences between how we learn to say "I heard a tree fall" and "I had a toothache."

Some time later I received the editor's letter to the author accompanied by the comments of two other reviewers (the reviewers were identified to each other but not to the author). One was Willard Day, who made a recommendation for revision similar to mine though on somewhat different grounds. Here is a brief excerpt from Day's review:

I think a serious weakness in the paper is the author's effort to account for this ambiguity in Skinner's writing in terms of the claim that Skinner has "changed his mind." I have little doubt that Skinner would deny the charge, showing that the author's move was tactically inappropriate to begin with. Besides, I believe that the claim is simply false. Skinner's thought has a complex kind of integrity, and as with all verbal behavior it remains under the control of complex contingencies.

The editor's letter concurred with our recommendations and accepted the manuscript conditional on revision (though to my knowledge the manuscript was never resubmitted and has not been published elsewhere). This was despite a stronger critique by the third reviewer, B. F. Skinner himself, whose recommendation nevertheless did allow for the possibility of resubmission. Skinner had written:

The paper is well written and appears to be well informed, except that there is a serious misunderstanding of Skinner's position. I don't think Skinner changed his views in 1945. Instead he developed further the emerging concept of stimulus control as a replacement for knowing or knowledge. . . . That [private stimuli] can come into control of verbal "reports" is the point of Skinner's article. That can happen only with the evolution of verbal behavior and the contingencies composed by a verbal community which induce the individual to respond in discriminative ways to stimuli which up to that point had served other physiological functions. Private events then remain inferences to the experimenter or philosopher, but they are just as directly observed by the person in whose skin they exist as any environmental stimulus.

Skinner acknowledged that private events could be causes, as implied by the statement that they can come into control of reports.

But he regarded them as only intermediate causes, and therefore less important than the initiating causes that were to be found in the environment: ". . . the private event is at best no more than a link in a causal chain" (Skinner, 1953, p. 279). Skinner had also argued that the language of private events is limited because consequences cannot be made contingent on the property of privacy (Skinner, 1953, p. 280). But should he have ruled out the possibility of generalization along the dimension of differential access by speaker and listener (e.g., from behaving privately by saying something while muting a telephone to behaving privately by instead saying the same thing to oneself)? The very extension of the vocabulary of privacy from one case to the other is evidence of such generalization. Skinner would certainly have answered such questions differently after he had written *Science and Human Behavior* than before.

Of course Skinner's verbal behavior had evolved over time. Some of the changes had been prefigured in the 1948 William James Lectures at Harvard that he was later to expand into the book *Verbal Behavior* (1957). Selection based on where individuals fall relative to the mean for a population may eventually shift the population so far that ancestral forms become rare or absent in it. Similarly, Skinner's account had shifted so much that the earlier forms were no longer recognizable in it. And just as fossil records are notoriously incomplete, Skinner's published work provided only intermittent samples of his evolving verbal behavior. It is therefore no surprise that the relation between his earlier and his later work was sometimes a topic of debate.

Faced with similar problems, biologists argued over whether evolution proceeded steadily or in jumps (Dawkins, 1986). Those who took one side or the other were respectively called gradualists or saltationists. But environmental contingencies can produce either type of evolution. If selection operates relative to a population mean, it is likely to progress gradually, but if new environmental niches are opened up after a geological catastrophe it may progress very rapidly. In the decade during which Skinner wrote *Science and Human Behavior* and *Verbal Behavior*, his own verbal behavior with respect to the application of behavior principles to human af-

fairs was a coordinated system that was ready to occupy a special niche within the verbal behavior of a culture.

CONSEQUENCES

So where are we now? Behavior analysts may readily acknowledge the impact of Skinner's work, but what of the larger culture surrounding its niche? Misrepresentations of behavior analysis still abound in undergraduate psychology textbooks. Skinner's accounts of thinking and of the language of private events are largely ignored by the cognitive science that so dominates much of contemporary psychology and related disciplines. His account of the self is often covered as a behavioral theory in personality textbooks, but in the absence of the prerequisites provided in *Science and Human Behavior* it is hard to see how even an accurate representation could be persuasive to many readers.

Skinner attributed the widespread resistance to the acceptance of behavioral interpretations of human behavior to their undermining of traditional concepts such as freedom and personal responsibility (e.g., Skinner, 1971). The systems of Copernicus and Galileo had displaced the earth from the center of the solar system to an orbit around the sun; Darwin had located the human species on a branch of an evolutionary tree rather than identifying it as a product of special creation; Freud had reduced the scope of human consciousness by characterizing it as merely one piece of a larger system that was mostly beyond awareness.

Undoubtedly these and some others mentioned earlier were not the only issues. For example, Skinner had relied on some terminology that was to become troublesome. "Implicit in a functional analysis is the notion of control. When we discover an independent variable which can be controlled, we discover a means of controlling the behavior which is a function of it" (Skinner, 1953, p. 227). This usage is not very far from that of experimental controls or control groups, but still the term is not sufficiently distanced from usages consistent with dominance and coercion, such as "being in control" (and consider the more contemporary "control freak"). "Control" is a controversial word, yet what were the alternatives? "Influence" is too weak, "cause" is vulnerable to philosophical chal-

lenge, and "determination," though perhaps accurate enough, invites debate over free will versus determinism.

It has not helped that some terms that began within behavior analysis have evolved a long way from their original usages. For example, too many parents and teachers speak of and use timeout though uninformed about its rationale and its side-effects, and the term "behavior modification" has recently been co-opted by groups that primarily use coercion and restraint to manage oppositional teenagers. The level of competence of the practitioners of such inappropriate applications is illustrated by their common acceptance of the erroneous but widespread myth that reinforcement has hidden costs (Eisenberger & Cameron, 1996).

Inevitably there were also some inconsistencies. For example, Skinner often slipped back and forth between the usage of reinforcing responses and that of reinforcing organisms, whereas K&S had been consistent in favoring the former usage, which forces the writer to be specific about the response that has been reinforced (Catania, 1987a). And some of Skinner's (1953) interpretations of human phenomena seemed to appeal to private states, as when, in discussing personification, he wrote: "The practice resolves our anxiety with respect to unexplained phenomena and is perpetuated because it does so" (p. 283). How do we interpret the term "want" in the following? "Regardless of how much we stand to gain from supposing that human behavior is the proper subject matter of a science, no one who is a product of Western civilization can do so without a struggle. We simply do not want such a science" (p. 7). No doubt Skinner could have elaborated on anxiety in terms of the effects of preaversive stimuli and want in terms of establishing operations, but he also recognized that for some audiences at some times a colloquialism might be more effective than a technical term.

With some other terms, difficulties arose because some who adopted them did not heed Skinner's cautions. For example, the concept of self-reinforcement has both empirical and logical difficulties (Bandura, 1976, 1995; Catania, 1975, 1976, 1995a). Skinner (1953) had pointed out, long before that controversy surfaced, that "The ultimate ques-

tion is whether the consequence has any strengthening effect upon the behavior which precedes it" (p. 238) and that "A mere survey of the techniques of self-control does not explain why the individual puts them into effect" (p. 240). And with regard to self-control, he wrote: "When we say that a man controls himself, we must specify who is controlling whom" (p. 229).

In these and in many other cases, such as Skinner's treatments of private events and of thinking and of seeing as behavior, the topic of verbal behavior hovers always close by in the background, so that *Science and Human Behavior* can also be regarded as a prelude to Skinner's other seminal book on human behavior, *Verbal Behavior* (Skinner, 1957). That book took up many of the lines of argument that the earlier one had introduced (Catania, 1997). Later, Skinner expanded upon those verbal processes in his discussion of what he called rule-governance (Skinner, 1969). Because the status of rules varies so much across different audiences, it is now sometimes less ambiguously called verbal governance (Shimoff & Catania, 1998). The analysis of verbal governance is concerned with how, given an appropriate history of social contingencies, behavior can be controlled by words even in the face of strong competing nonverbal contingencies (obeying orders in the military provides one obvious example; religious fundamentalism provides many others).

The behavior-analytic approach has not spread as much as Skinner might have wished. Yet as we survey the contemporary scene we can point to many applications traceable in one way or another to *Science and Human Behavior*. Some, in behavioral economics, in education, and in the behavior therapies, correspond to chapters in the penultimate section of Skinner's book. In the areas of government and religion, however, an impact is more difficult to detect. Skinner treated these two as separate topics, but both entail strong elements of verbal governance. Their analysis in that context is one of the urgent tasks that Skinner has left to us.

In the case of religion, Skinner made some appeal to accidental contiguities of events in his concept of superstition. But though the effects of accidental contiguities on behavior may occasionally be substantial, they are mostly quite temporary. Verbal governance

has far more powerful effects, and Skinner recognized that much human behavior called superstitious involves verbal practices rather than the accidental strengthening of nonverbal behavior. It is useful to distinguish the kinds of consequences that governmental institutions can bring to bear from those available to religious institutions. But in their reliance on verbal governance, the two sorts of institutions typically have much in common. Exhortations to behavior in the name of patriotism are not so very different from exhortations in the name of a deity.

Verbal competence is said to separate man from beast. It is said to make human rationality possible. It is therefore a curious cultural practice that allows more control to be exerted on behalf of religious belief than on behalf of scientific findings, because the criterion for religious belief is precisely that it is unverifiable. Freedom of religion implies that you can act on almost any belief whatsoever, unless what you believe happens to be supported by evidence. Science, and especially behavioral science, is at risk in any culture that regards contact with words as more fundamental than other kinds of environmental contact.

Skinner's *Science and Human Behavior* set the stage for such extensions of behavior analysis by including government and religion and other manifestations of verbal behavior within its purview. While continuing to offer explanations of behavior in its own terms, Skinner here turned from lever presses and key pecks to looking, attending, thinking, imagining, and other sorts of behavior that cannot easily be defined in terms of the movements of muscles or the secretions of glands. In so doing, he extended the boundaries of what counts as behavior and what is within the reach of its science (cf. Laties & Catania, 1999, p. xxv). An operant interpretation of verbal behavior was crucial to his extensions to governing and obeying and believing and praying. These varieties of behavior still call for further analysis. If the reactions to Copernicus and Galileo and Darwin and Freud are anything to go by, many of the messages still to be derived from the later chapters of *Science and Human Behavior* will not be well received.

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